

places and monuments illustrative of various countries. Altogether the work is a really good specimen of its kind. Another volume will bring the story down to the present time.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Tide Predictor

I SEE in your last number (p. 467), among the editorial notes, the following:—"Mr. Roberts of the *Nautical Almanac* office is authorised, by resolution of Council of the Secretary of State for India, dated August 7, 1880, to make it generally known that his Tide Predictor may be employed for the preparation of Tide Tables for any port for which the requisite data are forthcoming."

I think it right to call your attention to the fact that the Tide Predictor is in no sense of Mr. Roberts's invention or design. He was employed in 1873 by me, as chairman of the British Association Tidal Committee, to calculate the number of teeth in the wheels of the first Tide Predictor (now the property of the British Association, permanently deposited in South Kensington Museum), and to superintend its construction in London by Messrs. A. L  g   and Co. The second Tide Predictor was made for the India Office, according to my advice, by Messrs. A. L  g   and Co. of London, under the superintendence of Mr. Roberts. In respect to the plan of the wheelwork, which is wholly due to Messrs. L  g  , it is a copy of the first instrument. It is an improvement on the first instrument in having twenty tidal components instead of ten, and in having the well-known rigorous method of the slide (Thomson and Tait's "*Natural Philosophy*," § 55, or "*Elements of Natural Philosophy*," § 72) for producing simple harmonic motion in a straight line from circular motion, instead of the approximate method of pulleys centred on crank-pins, which for simplicity and economy I used in the first instrument.

WILLIAM THOMSON

The University, Glasgow, March 19

The Magnetic Storm of 1880, August

THE Astronomer-Royal has handed to me a copy of the photographic record of the variations of magnetic horizontal force as registered at Toronto during the disturbed period of August 11 to 14 last. The records of declination and vertical force were imperfect and have not been received.

The comparison of the Greenwich and Zi-ka-wei (China) curves for the same period (*NATURE*, vol. xxiii. p. 33) indicated that the commencement and end of disturbance (especially the commencement) occurred nearly simultaneously at both places, and this circumstance is now further corroborated by the Toronto horizontal force curve.

In what follows, the reference throughout is to Greenwich time.

The disturbance at Toronto commenced on August 11 at 10.20 a.m. At Greenwich (*NATURE*, vol. xxiii. p. 33) it commenced also at 10.20 a.m., and at Zi-ka-wei at 10.16 a.m.; at Melbourne (*NATURE*, vol. xxii. p. 558) it commenced at 10.33 a.m.

Disturbance ceases at Toronto at about midnight of August 11, and at Greenwich and Zi-ka-wei also at about or near midnight, but it dies out more or less gradually, not allowing the limit of disturbance to be always very precisely fixed.

Sudden motion is again shown (after some hours of quiet) at Toronto on August 12 at 11.40 a.m.; also at Greenwich at 11.40 a.m.; some minutes sooner at Zi-ka-wei; and at Melbourne at about 11.38 a.m.

Disturbance again dies out more or less gradually at Toronto on August 14 about 7 a.m.; at Greenwich and Zi-ka-wei at about 6 a.m.; and at Melbourne at about 7 a.m.

The commencement of disturbance in the above instances is definite, and the agreement in time, considering the widely-separated geographical position of the four places concerned, is

noteworthy. The cessation of disturbance is less definite, as has been already remarked, but even here the discordance in time is not very wide.

WILLIAM ELLIS

Royal Observatory, Greenwich, March 12

Prehistoric Europe

I MUST adhere to my decision not to play the part of *Secutor* any further to a glacial *Retiarius* in the arena of *NATURE*. If his net be strong enough to carry the Upper Pleiocene and the Pleistocene mammalia of Europe, as well as Pal  olithic man and the Neolithic skull of Olmo, I wish him joy of them. If, further, he will kindly give me the proof that the mammalia of Auvergne, considered Upper Pleiocene by Falconer, Gaudry, Gervais, and other leading pal  ontologists, are, as he terms them, "a hash up," they shall be properly served and *iced*, if necessary, in my second edition.

I feel however that it is only right for me to notice the new gladiator who springs to the aid of his friend. The antiquity of man in the Victoria Cave is solely due, as it appears to me, to the *perfervidum ingenium* (I speak in all respect) of Mr. Tiddeman. It was first based on a fragment of fibula which ultimately turned out to belong to a bear. Then it was shifted to the cuts on two small bones, which were exhibited and discussed at the British Association, at the Anthropological Institute, and at the Geological Society of London. The bones are recent, and belong to sheep or goat, two domestic animals introduced into Britain in the Neolithic age. The cuts have been probably made by a metallic edge. Numerous bones of the same animals, in the same condition and hacked in the same way, occurred in the Romano-British refuse-heap on the top of the clay, and frequently slipped down over the working face to the bottom of the cutting before I resigned the charge of the exploration to Mr. Tiddeman after nearly four years' work. There were frequent slips afterwards. Under these circumstances the reader can decide whether it is more probable that the mutton-bones in question did slip down from a higher level to be picked out at the bottom, or that there is evidence of "interglacial" (J. Geikie) or "preglacial" (Tiddeman) man possessed of domestic animals and probably using edged tools of metal. The mutton-bones seem to me to prove so much on the latter hypothesis, that they may be thrown aside without further thought.

The reindeer (bones of feet) was found in 1872 along with fox, rhinoceros, elephant, hy  na, and bison in the cave at the lower horizon, which afterwards was proved to contain the hippopotamus. It was omitted in Mr. Tiddeman's lists up to 1876, when I called his attention to the fact. Then he wrote that the fact that it was so found was "noteworthy," and that "these remarks [his generalisations] were made solely on the evidence which passed through your present reporter's hands since he undertook to conduct the exploration of the cavern" (*Brit. Ass. Rep.*, 1876, p. 118). Surely it is too late, in his letter to *NATURE* (March 10, 1881), to recall this on the grounds that these remains were discovered in a shaft, that my exploration was not carried on so accurately as his own, and further, that because he did not find the reindeer in the lower strata that I did not. It is not for me to compare my own experience in cave-hunting with his, or to point out the value of negative evidence. The exploration while under my charge was *not* carried on by shafts only. When the hy  na-layer was reached it was followed in the deep cutting visited by the British Association in 1873. The presence of reindeer in the hy  na-layer renders Mr. Tiddeman's views untenable which are based on its assumed absence. Most of these points have been so fully argued out before the above-mentioned societies, that I am sorry to be obliged to repeat them in this letter.

W. BOYD DAWKINS

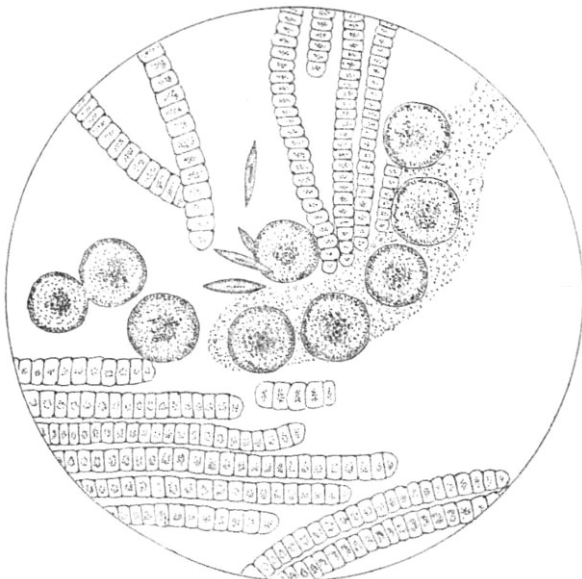
Owens College, March 11

Oceanic Phenomenon

H.M.'s surveying ship *Alert* was recently engaged in searching for a "shoal" which was reported as existing some 200 miles to the southward of Tongatabu, in the South Pacific. In the course of the survey—which I may add tended to disprove the existence of any such shoal—it was observed that for several days the sea-surface exhibited large discoloured patches, due to the presence of a fluffy substance of a dull brown colour, and resembling in consistency the vegetable scum commonly seen on the stagnant water of ditches. This matter floated on the surface in irregular streaky patches, and also in finely-divided particles impregnated

the sea-water to a depth of several feet. Samples for examination were obtained by "dipping" with a bucket, as well as by the tow-net. It seemed to be a Confervoid Alga.

On slightly agitating the water in a glass jar, the fluffy masses broke up into minute particles, which, under a magnifying power of sixty diameters, were found to be composed of spindle-shaped bundles of filaments. Under a power of 500 diameters these filaments were seen to be straight or slightly-curved rods, articulated but not branching, and divided by transverse septa into cylindrical cells, which contained irregularly-shaped masses of granular matter. These rods, which seemed to represent the adult plant, measured $\frac{1}{2000}$ inch in width. On carefully examining many samples, some filaments were detected, portions of which seemed to have undergone a sort of varicose enlargement, being more than twice as wide as the normal filaments. These propagating filaments (if I am right in so calling them) were invested by a delicate tubular membrane, and contained some granular semi-transparent matter, in which was imbedded a row of discoid bodies; the latter appearing as if about to be discharged from the ruptured extremity of the tube. These bodies measured $\frac{1}{1000}$ th of an inch in diameter: when seen edgewise presented a



lozenge-shaped appearance, and were devoid of cilia or striæ. Conjugation was not observed.

On allowing a jarful of the sea-water to stand by for twenty-four hours it was found that the confervoid matter had all risen to the surface, forming a thick scum of a dull green colour, while the water had assumed a pale purple colour, resembling the tint exhibited in a weak solution of permanganate of potash.

From November 24 to 29, during which time the ship traversed slowly a distance of 330 miles, the sea contained these organisms. For the first three days the large patches were frequently in sight, and during the rest of the time the surrounding water presented a dusty appearance from the presence of the tiny spindle-shaped bundles. On the evening of the 26th an unusually dense patch was sighted and mistaken for a reef, being reported as such by the look-out-man aloft.

Sydney, January 24

R. W. COPPINGER

Feeding a Gull with Corn

IN Prof. Semper's recently-published work on the "Conditions of Existence as they Affect Animal Life," a review of which from the pen of Prof. Lankester appeared in your columns a fortnight ago (vol. xxiii. p. 405), allusion is made on pp. 67, 68, and elsewhere to John Hunter's celebrated experiment of feeding a gull with corn. Prof. Semper, however, seems not to have been aware of the precise nature of the result of Hunter's experiment. He says: "The English anatomist Hunter purposely fed a sea-gull for a whole year on grain, and he thus succeeded in so completely hardening the inner coat of the bird's stomach,

which is naturally soft and adapted to a fish diet, that in appearance and structure it precisely resembled the hard, horny skin of the gizzard of a pigeon."

The original account, I believe, of Hunter's experiment, was published in Sir Everard Home's "Lectures on Comparative Anatomy" (vol. i. p. 271, 1814), and an extract from that work is appended to the description of Hunter's original preparation, still preserved in the College of Surgeons, in the descriptive catalogue of that collection (vol. v., 1833, pp. 149-50, Prep. 523). What Hunter succeeded in effecting was to very much increase the thickness of the muscular walls of the gizzard, which, as may be seen by comparing his specimen (No. 523) with that of the stomach of another gull close by, have become developed to an extent about double their usual size. There is no manifest increase in the thickness of the "inner"—or so-called "epithelial"—coat of the stomach visible in the preparation, nor do Home or Owen allude to any such feature in their descriptions. Hunter's experiment, therefore, simply comes under the numerous well-ascertained instances of the increased development, consequent on increased use, of muscle, and has no real connection with the "modifying effects of food," such as that produced in canaries by feeding them on cayenne pepper, and others cited by Semper.

W. A. FORBES

Zoological Society's Gardens, N.W., March 18

Dynamics of "Radiant Matter"

AS the chief object of Mr. Preston's paper under the above title in NATURE, vol. xxiii. p. 461, seems to be to support Le Sage's "shelter theory" for gravity, you will perhaps let me point out one objection to that theory in any form which has hitherto been deemed conclusive, and with which Mr. Preston does not deal. It is that under it gravity would not vary, as it is known to do, equally with mass, but would vary *not* equally.

The theory applies perhaps so long as you consider only the case of isolated atoms, but it fails entirely when applied to clusters of atoms.

Observation shows that gravity varies only with distance and with mass; but if it were caused by any form of shelter hitherto imagined, it would vary also with density and with bulk in such a way that a pound of, say water, would weigh more than a pound if raised into steam, because its atoms, in loose order as steam, would give each other less shelter from the action of the kinetic æther than when in close order as water, and in such a way also that two spheres of, say iron, each weighing one pound, would weigh less than two pounds if welded into one sphere, because some atoms in the one sphere would be better sheltered than any atoms in the two spheres.

WM. MUIR

March 21

The Oldest Fossil Insects

MR. S. H. SCUDDER has published (Anniversary *Memoirs* of the Boston Society of Natural History, 1880, pp. 41, plate 1) a memoir on the Devonian Insects of New Brunswick. The fragments of the six described species were discovered by the late Prof. C. F. Hartt in 1862, and have been since 1865 described in several papers by the same author. The new paper is a very detailed and elaborate one, with entirely new and improved figures, and is followed by a number of conclusions, as the final result of his work (Report, *Amer. Journ. of Sci.*, Feb. 1881). The conclusions would be of prominent importance for the history of the evolution of insects, if they could be accepted without reserve. Of course facts and conclusions should be able to stand the most severe test; and that is not the case with this publication. "As the simpler Devonian insects have certain special relations," he says, "with the Ephemeridæ, their description is preceded by an account of the wing-structure of the modern Mayflies as a basis of comparison" (p. 4).

The simple fact that not one of the described species has any relationship to the Ephemeridæ is sufficient to cause us to object to his descriptions and conclusions related to this family. This statement is not based upon a difference of opinion, but simply on the evidence of facts which cannot be denied by any one conversant with the families Ephemeridæ and Odonata.

Platephemera antiqua is a part of the apical half of the wing, without the tip, of a gigantic dragonfly. The suddenly narrowed second cubital space is to be found in *Isophlebia* of the Solenhofen slate. The imperfection of the fragment allows no further conclusions.

Gerephemera simplex is a diagonal fragment of the middle of